A REVIEW OF

"PROF. REESE'S REVIEW" OF THE WHARTON TRIAL.

WITH

A BRIEF NOTICE OF THE SCHOEPPE TRIAL.

BY

PROF. WILLIAM E. A. AIKIN, M. D., LL. D.,
PROFESSOR OF CHEMISTRY, ETC., IN THE UNIVERSITY OF MARYLAND

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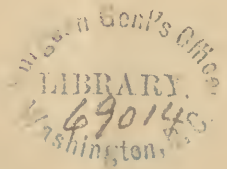

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A REVIEW OF "PROF. REESE'S REVIEW" OF THE WHARTON TRIAL, WITH A BRIEF NOTICE OF THE SCHOEPPE TRIAL.

After the Wharton trial, on the charge of murder, was concluded, there appeared in the April number of Dr. Hay's *Journal*, a review of the testimony, written by Prof. Reese, connected with one of the medical schools in Philadelphia, and one of the witnesses for the defence. That review, so ungentlemanly, so unprofessional, so illiberal, so unjust, so untrue, has already been exposed, in its medical aspects, by the medical gentlemen attacked. What Prof. Chew and Dr. Williams have done for the medical questions discussed, I desire to do for the chemical questions. Every one knows to what lengths a mere lawyer, especially if he is wanting in all gentlemanly instincts, will go to serve his client. No one is, therefore, surprised if such a one perverts the meaning of testimony to suit his own purposes. His effort is to secure a verdict, and all means are considered lawful for that end. But dispassionate men of science should look at things from a different stand-point, and be able to distinguish between truth and error.

Instead of following the statements of the "Review" *seriatim*, it will save time and space to state concisely the evidence offered by the State, to show the presence of antimony in the stomach of General Ketchum, and then to note the objections to the processes employed. We will leave out, then, all question of things sought for but not found, and confine our-

selves to the treatment of portion "B," to determine whether or not antimony was present. Portion "B" represented a part of the original contents of the stomach of General Ketchum, altered in condition only in having been examined for vegetable alkalies. The first step was to acidulate this fluid with tartaric acid, to filter it, and to examine the filtrate with sulphuretted hydrogen gas. This proceeding is severely condemned, inasmuch as the organic matter was not first disposed of. How far the removal of such matters is really essential, we may gather from acknowledged authority. Some advocate, as a preliminary step, a very partial removal of organic substances. Turner, under the head of "Antimony," says: "The detection of antimony in mixed fluids, as when tartar-emetic is mixed with articles of food, is conducted in the following manner: The substances are first digested in water, acidulated with about a drachm of hydrochloric and tartaric acids, which coagulate some organic matters, and give complete solubility to the oxide of antimony." Christison, under the head of "Antimony," "Process for Tartar-Emetic in Organic Mixtures," says: "If the subject of analysis be not already liquid enough, add distilled water. Then acidulate with a little hydrochloric and tartaric acids, the former of which throws down some animal principles, while the latter dissolves all precipitates formed with tartar-emetic by reagents or organic principles, except the sulphuret." Other authorities ignore even such partial removal of organic matter. Brande, in chapters on Antimony, "Analysis in Cases of Poisoning," says: "The contents of the stomach, or the coats of the organ, may be boiled in hydrochloric or tartaric acid, and the filtered liquid subsequently decomposed by the gas" (sulphuretted hydrogen). In "Taylor on Poisons," under the head of "Detection of Antimony in Organic Liquids," we read: "The organic liquid should be filtered and then strongly acidulated with tartaric acid. A current of sulphuretted-hydrogen gas is now passed into it until there is no further precipitation. The sulphuret is collected, washed, and dried; if it be the sulphuret of antimony, it will have an orange, red, or brown color, and will, when dried, be dissolved by a small quantity of boiling muriatic acid (forming sesquichloride of antimony) with the evolu-

tion of sulphuretted-hydrogen gas. The boiling should be continued for several minutes. On adding this solution to a large quantity of water, a dense white precipitate of oxychloride of antimony falls down. This is characteristic of antimony. If it be objected that nitrate of bismuth undergoes a similar change when poured into water, hydrosulphuret of ammonia will easily enable the operator to distinguish the two metals; the antimonial precipitate is turned of an orange-red color by that solution, while the bismuthic precipitate is turned of a deep black. The antimonial precipitate is easily dissolved by tartaric acid; that of bismuth is not." In his work on "Medical Jurisprudence," Taylor gives the same process in almost the same words. In chapter on "Tartar-Emetic, Chemical Analysis in Liquids containing Organic Matter," he says: "The liquid acidulated with tartaric acid should be filtered, and a current of sulphuretted hydrogen passed into it until there is no further precipitation." All the peculiar characteristics of antimony, as enumerated by Taylor, were obtained in the most unequivocal manner from the material under examination—that is, from the contents of the stomach of General Ketchum. The liquid was acidulated with tartaric acid, filtered and treated with sulphuretted-hydrogen gas; the precipitate thrown down was collected, dried, and dissolved in boiling hydrochloric acid. This acid solution, added to water, gave a white precipitate, which appeared as a white cloud as long as it remained suspended. That white cloud dissolved entirely in tartaric acid, and also became bright orange-red on the addition of hydrosulphate of ammonia. Now, unless there is some one thing known which, when treated in exactly the same way, will give exactly the same results, there seems to me no escape from the conclusion that antimony must have been present in the contents of the stomach examined. It will not do to say there may be other things (things yet unknown) which might give the same results if they were present, and therefore no one can be sure that the results obtained were really produced by antimony, since they might all have been produced by this supposed something else, "without an atom of antimony present."

As our business is not to discuss possibilities, but to deal

with facts, such argument needs no refutation. The case is somewhat different when the lawyer for the defence, during the cross-examination of the writer, elicited the fact that "lead would sometimes give a red precipitate which might be mistaken for the antimonial red," ingeniously leaving the jury to infer that the red precipitate obtained might not have been antimonial, after all—might have been lead. This was all very well, and no more than was to be expected from a lawyer who goes in to win, *per fas aut nefas;* but I was a little surprised and sorry to find this statement quoted in the "Review," in a connection which leaves an uninstructed reader in danger of drawing the same wrong inference. "He" (Dr. Aikin) "then experimented with portion B, being led to suspect that antimony was present from the reactions of portion A. He testified that there could not have been present (in portion A) any metal which gives a black or dark sulphide, or the precipitate would have been black. But he admitted, in the cross-examination, that lead would sometimes give a red precipitate which might be mistaken for the antimonial red." If the red color of the precipitate might have been produced by lead, because lead compounds, by certain treatment, will give a red precipitate—by the same reasoning, copper, or iron, or mercury, or any other of the various metals which give red precipitates, might have been present. It ought to be obvious to every one that, before the red compound of any other metal can be mistaken for an antimonial compound—first, it must be produced by exactly the same process; and, second, when produced it must exhibit identically the same physical and chemical characteristics as those relied upon to distinguish antimony from all other known substances. A good deal of virtuous indignation has been expended on the "novel if not unique" method pursued for determining quantities. Why this method was pursued becomes intelligible when the reasons, as given in evidence, are quoted in connection: "I have never attached any importance at all to the question how much was found in the stomach, except in cases in which things (that might accidentally get into the stomach) are present in very minute quantities." If tartar-emetic had been prescribed for the deceased, the presence of a fraction of a grain

in his stomach after death would have been no proof of antimonial poisoning. But where no antimonial remedy is prescribed or given, as in the case of General Ketchum, then the presence of any quantity is a "suspicious fact, and requires explanation." It seems to me self-evident that the portion of poison found in the stomach after death is not that which has caused death, but is merely the residue left after the action of so much as was necessary to produce death. That portion which produces death, being absorbed into the system, must consequently disappear from the stomach. Therefore, the exact weight of the antimony in the stomach of General Ketchum seemed entirely unimportant, and was given only as a conjecture, or rather as an opinion founded on a conjecture. It was not the particular weight of antimony found that could influence the case, but the fact that any at all was found. While speaking of exact weight, we may notice one rather sensational mistake of the reviewer, where he states: "In analyzing the contents of the tumbler, Prof. Aikin adopted the same process that he had employed in the examination of the stomach, using—however, *only one grain* of the material." The professor himself stated on the witness-stand, in regard to the sediment in the tumbler, "About one-half of the original quantity was treated with sulphuretted hydrogen, and gave me a precipitate of orange-red sulphuret of antimony;" and, subsequently, he stated that "fifteen grains were, speaking within bounds, present," as the whole weight of the sediment. I would here observe, in reference to this same tumbler, that one important fact seems to have been overlooked. As the contents of the tumbler gave the same evidence of the presence of antimony as the contents of the stomach, and as it has never been pretended that either chloral or gelseminum was present in the tumbler, if there was no antimony there, then the milk, or sugar, or liquor, either brandy or whiskey, must have produced the orange-red precipitate. And this, if a fact, introduces new elements of uncertainty in medico-legal examinations. But who will believe it to be a fact?

The tumbler referred to was found in the house where General Ketchum died, under circumstances which connected it with the case. It had contained milk-punch, which, poured out,

left behind it a sediment containing a large percentage of tartar-emetic, unless my chemical tests could deceive me. The defence were asked whether milk, or sugar, or liquor, could give results resembling antimony, and the reply by one was, that they might; by another, that he did not know, as he had never tried them.

It is objected by the reviewer that other and more delicate processes were not employed, and several are named, the production of the metal and others. To say nothing of the fact that the metal was afterward produced by Prof. Touey from the body of the deceased, although that fact was withheld from the jury on a technical point, I cannot concede that an analytical chemist, in searching for any thing, can be required to make use of all the processes that have ever been devised for the object in view. He must satisfy his own mind, so that there shall not remain even the shadow of a reasonable doubt. He must be perfectly satisfied that his conclusions are perfectly certain. Short of that he cannot stop; beyond it he need not go, since there can be no degrees of certainty.

"It is admitted by one or two of the witnesses for the defence, that if Dr. Aikin had been operating on a metal, and knew it to be a metal, the three tests he used would constitute a very strong probability that it was antimony." To warrant that conclusion, the compounds formed by the action of these tests must be identified by their physical and chemical characters as true antimonial compounds, otherwise they could never prove the metal in question to be antimony. But, if they are recognized and identified as true antimonial compounds, then, whenever they can be produced, they must be equally sufficient to prove the presence of antimony, even where one is not operating on a known metal. The last point that requires notice is contained in the statements that "the chemical testimony for the defence most completely demonstrated that the color-test, the orange-red precipitate, with all its subsequent reactions, save in one single point, might be exactly imitated without the presence of an atom of antimony;" and, "it remained for Prof. McCullough to develop the interesting and important fact that, if the organic substance in the solution

acted upon by the sulphuretted hydrogen be the tincture of yellow jessamine and chloral (the latter is not essential), and sufficient time be allowed (say, several hours), the color of the precipitate is of a decided orange-red. This effect may be speedily brought about by dropping sulphide of ammonia into a slightly acidulated solution of the jessamine-tincture and chloral. The color of the precipitate here very soon becomes a rich orange-red, which would defy any chemist to distinguish from the supposed characteristic orange-red of sulphide of antimony. . . . The fact of real importance and interest to the toxicologist is, that there are other matters besides antimony which will give the characteristic orange-red precipitate." No one, we imagine, will maintain that the mere fact "that there are other matters besides antimony which will give the characteristic orange-red precipitate," is of any importance unless it can be proved that these "other matters" will give exactly the same results as an antimonial solution when all are treated in exactly the same way. It is hardly to be supposed that "Prof. McCullough's experiments, repeated and verified by Dr. Genth and by Profs. Reese and White," were merely intended to show that, while antimony, treated in one way, would give an orange-red precipitate, chloral and gelseminum, when treated in a widely-different way, would give an apparently similar result. Even if this were a fact, which it is not, it could have no bearing on the question. Surely nobody can suppose that any one thing is liable to be mistaken for another, unless both when treated in the same manner will give similar results. Therefore, the "experiments of Prof. McCullough, repeated and verified by Dr. Genth and by Profs. Reese and White," must have been intended to prove that "certain matters," as choral and gelseminum, when treated in exactly the same way as an antimonial solution, would give results so similar to those given by the antimonial that they might easily be mistaken; and, therefore, proof of the presence of antimony might be "exactly imitated without the presence of an atom of antimony." In January last I was called upon by the law-officers of the State to examine and verify or disprove this proposition. According to my instructions, I was required to investigate the char-

acter of the singular deposit exhibited by Prof. McCullough, in the court-room, to ascertain the conditions required for its production, and to determine its properties, physical and chemical, sufficiently to contrast it with a true antimonial deposit. My task, therefore, was not merely to ascertain whether, by any kind of treatment, chloral and gelseminum alone, or in conjunction with certain organic matters, could be made to yield a precipitate resembling an antimonial precipitate, but rather to ascertain whether those substances could give a result in any way resembling that obtained from an antimonial solution when all were treated in exactly the same way. The materials used by Prof. McCullough, as far as I recollect, were albumen, milk serum, pepsin, lactic acid, beef-tea, chloral-hydrate, tincture of gelseminum, and tartrate of soda, with excess of tartaric acid. These were all examined, at first separately and then collectively, by the same reagents applied in exactly the same way as in the examination of the contents of the stomach of General Ketchum. That is, the several solutions were first made distinctly acid to test paper by the use of tartaric acid, and then sulphuretted-hydrogen gas was passed through to saturation. In not a single instance could I get any deposit at all resembling in the most remote degree a true antimonial deposit. In not a single instance could I get a bright or even deep-colored precipitate. All the precipitates were white, or grayish white, or dingy white, except in the case of the tincture of gelseminum, where the yellowish color of the tincture gave a yellowish tinge to the solution, and apparently to the deposit seen through the surrounding colored liquid. To another portion of each of the same solutions I then added a few drops of a solution of tartar-emetic, when the sulphuretted hydrogen instantly produced a characteristic antimonial precipitate in each. It then became evident that the deposit which Prof. McCullough and his associates obtained from chloral and tincture of gelseminum could not have been obtained if the conditions of their experiments had been identical with those observed during the experiments on the contents of the stomach of General Ketchum. To the next inquiry, "How did Prof. McCullough get his deposit?" my results answer that free ammonia was necessary. The same acid solution of

chloral which gives no deposit when sulphuretted hydrogen is passed through it, will give an immediate deposit if ammonia is added in excess before the gas is used; or if, after it is saturated with the gas, ammonia is then added; or, lastly, if, instead of using sulphuretted-hydrogen gas, the experimenter uses sulphide of ammonium in its ordinary condition, containing an excess of ammonia. To obtain the chloral red deposit, free ammonia is then necessary. Not merely a free alkali, for potassa and soda, when substituted for ammonia, would not give equally satisfactory results. Neither will the presence of a salt of ammonia suffice. Sulphuretted hydrogen failed to give any other than the light-colored deposits above named if the solutions examined contained free tartaric acid, even when ammonia was present in combination. The solutions already named were next examined with sulphide of ammonium as a precipitant. With this reagent I could not get any precipitate presenting even any equivocal aspect from any of the solutions, with the exception of those of chloral-hydrate and tincture of gelseminum. From these, when mingled, and from the chloral by itself, the sulphide of ammonium threw down a deposit very closely resembling that exhibited by Prof. McCullough before the jury. When the sulphide of ammonium was added to a solution of chloral-hydrate containing no free acid, the liquid speedily became colored with a reddish-orange tint, and threw down a precipitate which, viewed through the colored liquid, showed a very suspicious resemblance to an antimonial deposit. But this resemblance vanished at once, and totally, when the deposit was collected on a filter, and its true color became visible. All the coloring-matter passed through the filter, with the filtrate, leaving on the paper a dingy-white deposit, which no imagination could liken to the orange-red precipitate produced from an antimonial solution. The apparent color of the deposit, as seen through the colored liquid, was merely an optical illusion. In this, as in all other cases, the true color of a precipitate cannot be determined unless seen in a colorless liquid, or in a dry state. The next inquiry was, whether the deposit from the chloral and gelseminum solutions exhibited any resemblance in chemical characters when contrasted with an antimonial

precipitate. The antimonial sulphide I found freely soluble in boiling strong hydrochloric acid with the evolution of sulphuretted-hydrogen gas. The chloral deposit I found very sparingly soluble. The resulting acid solutions, in both cases thrown into water, gave a white precipitate, or, when in small quantity, gave a white cloud, which, in the case of the antimonial compound, was perfectly soluble in tartaric acid. The white deposit from the chloral compound was insoluble in tartaric acid. The antimonial white compound became distinctly and permanently orange-red when treated with sulphide of ammonium. The chloral white compound, treated with sulphide of ammonium, gave at the time a somewhat similar color, but it has proved fugitive. Some of the phials in which the results described were enclosed last January are now before me. In some, the decomposition of organic matter has changed the appearance. All the antimonial precipitates retain their characteristic color, very little changed. But all the chloral and gelseminum deposits have lost the last traces of any resemblance to any antimonial product. Thus it will be seen that the precipitates supplied by chloral hydrate and tincture of gelseminum, when properly examined, cannot in any way affect the reliability of those tests on which the State depended to prove the presence of antimony in the stomach of General Ketchum. Those tests were the production of a more or less bright-colored or orange-red precipitate when sulphuretted-hydrogen gas was passed into the suspected solution previously acidulated with tartaric acid; the solubility of this precipitate in strong boiling hydrochloric acid; the decomposition of such acid solution with the production of a white precipitate, or a white cloud, when the solution was dropped into water; the entire solubility of this white cloud in a solution of tartaric acid, and the conversion of the white cloud into an orange red sulphide when treated with sulphide of ammonium.

Are these sufficient to demonstrate the presence of antimony beyond all reasonable doubt? They must be, unless something else is known besides the antimonial compounds which, treated in the same way, will give similar results. All the chemical witnesses for the defence were asked, "Is any such substance known?" and all, I believe, replied that

there was nothing known, except one witness, and his answer was qualified by stating there might be something, but he did not know of any thing. If any of the witnesses had stated that something besides antimony was known which could give all the results alluded to, he would surely have been asked what that something was. It seems to me that none of the organic matters tried can be this something, since none of them give any decidedly colored precipitate when treated with sulphuretted hydrogen in the presence of free tartaric acid, not even chloral or gelseminum. And, unless so treated, no contrast can be drawn. When Prof. McCullough experimented with chloral and gelseminum to show how easily they can be mistaken for antimony, and when Dr. Genth and Profs. Reese and White "repeated and verified" these experiments, were these gentlemen aware of the necessity for having free tartaric acid present in the solution they employed? If they were not aware of this they certainly overlooked a most important, a vital point. To maintain that a given process for detecting antimony is unreliable, because by another and widely different process something else can be made to yield apparently similar results, is very clearly a "*non sequitur.*" Whenever any thing is found (except an antimonial compound) which, subjected to exactly the same treatment, will give all the results I obtained from the materials I used, it will be time enough to say, "there might have been something else present which was mistaken for antimony." How did Prof. McCullough's chloral precipitate agree in physical characters with the one I obtained? Unless the professor insists on calling his white deposit orange-red, merely because it was suspended in an orange-red liquid, there was no resemblance in color. His precipitate, apart from the deep-colored liquid, collected on a filter, was a dingy white; mine, viewed in the same way, was a shade of red. Again, there was an equally fatal discrepancy in chemical characteristics—in the action of tartaric acid on the white deposit produced by adding the hydrochloric-acid solution of the first precipitates to water—the one dissolved in tartaric acid, the other was insoluble. From all this, it would appear that the deposits from chloral and gelseminum, as obtained by Prof. McCullough's

process, "repeated and verified" by Dr. Genth and Profs. Reese and White, differ from a true antimonial deposit: 1. In the mode of production—the one is thrown down from an acid liquid, the others only from an alkaline liquid; 2. They differ in appearance—one is a shade of red, the other a shade of white; 3. They differ chemically in their behavior with tartaric acid—the derivative of one is soluble, and that of the other is insoluble, in the vegetable acid. And it would seem safe to conclude that two things which differ in their mode of production, which differ in appearance when produced, and which differ in at least one important chemical character, are not easily to be mistaken for one another—certainly not by a practised analytical chemist. I have lately revised, in part, the work done last winter, using, however, only solutions of chloral and gelseminum. Sulphuretted-hydrogen gas was passed through these solutions separately, and also through a mixture of the two, to which a few drops of a solution of tartar emetic had been added. The three liquids were examined in different conditions: 1. Acidified with tartaric acid until they reddened blue litmus-paper; 2. Made alkaline by adding aqua ammoniæ until red litmus-paper was changed; 3. Without the presence of either free acid or free alkali, which I will call neutral. The acid and the neutral solutions of chloral and gelseminum behaved exactly alike. Sulphuretted hydrogen threw down a white precipitate from the chloral, a less abundant white precipitate from the gelseminum, evidently colored by the coloring-matter of the tincture, and a vivid, orange-red precipitate from the mixed solutions containing antimony.

The alkaline solutions behaved very differently—the chloral solution speedily became colored, at first brownish, then reddish, then orange-red, and, ultimately, very dark, with an abundant precipitate appearing colored in the colored liquid, but dingy-white when separated. The alkaline gelseminum solution gave a very sparing precipitate, very light-colored, nearly white, the liquid assuming a yellowish tinge. The glasses in which these reactions took place have now been standing nearly four days, without any very obvious change. Still another irreconcilable difference exists between my experiments and those of Prof. McCullough, "repeated and veri-

fied by Dr. Genth and Profs. Reese and White." Their experience was, that, when treated with sulphuretted hydrogen in the manner already detailed, "tincture of yellow jessamine and chloral (the latter is not essential), and sufficient time be allowed, say, several hours, the color of the precipitate is of a decided orange-red." My experience is somewhat different. With me, the tincture of jessamine, which in the above extract is claimed to be essential, is not only not essential, but is absolutely inactive. In my hands, the tincture of yellow jessamine by itself, and by any treatment I have employed, cannot be made to yield any orange-red precipitate. To determine the influence of time, I passed a stream of sulphuretted-hydrogen gas through a solution of the tincture of yellow jessamine acidulated with tartaric acid, during "several hours," six hours continuously, and could not get the faintest trace of any orange-red precipitate. On the other hand, the chloral, so far from being "not essential," is absolutely necessary for the production of the apparently colored, but really nearly colorless, precipitate already described. And this precipitate, instead of requiring "several hours" for its appearance, requires less than several minutes.

It ought not to appear strange to any one that, with these facts before me, I am driven to the conclusion that "Prof. McCullough's experiments, repeated and verified by Dr. Genth and Profs. Reese and White," involved sources of error unsuspected and unnoticed.

Assuming that these gentlemen had but one object in view, assuming that they were honestly endeavoring to ascertain the truth, and to make that truth apparent to the jury, their evidence suggests a few inquiries. The "Review" says: "To rely upon this color-test alone" (the orange-red precipitate in the search for antimony), "might be attended with the most serious and fatal mistake." Well, who denies the truism? Who does rely upon this color-test alone? Did any of the witnesses for the State swear that they would rely upon this color-test alone? If so, who were they, and what did they say? The "Reviewer" does not say that any of them did, but he insinuates that some one did. He did not dare to make the charge openly, but did choose to make it covertly. How does this consist with an honest endeavor to establish the

truth? Again, when the witnesses for the defence testified "to the similarity in appearance between the precipitated sulphide of antimony, and the precipitate obtained by passing sulphuretted hydrogen through just such a mixture as was found in the stomach of General Ketchum, after treating it with hydrochloric acid, the color in both cases being orange-red," did they mean to say that the process described was identical with that employed by Dr. Aikin in his treatment of "Portion B," in which antimony was sought? Dr. Aikin's testimony was, "that to 'Portion B' he added an excess of tartaric acid, and then, with sulphuretted hydrogen, got a precipitate of a brownish-red color." The "Reviewer" could not then venture to say explicitly that the two processes were identical, and yet leaves this false inference to be drawn by the reader. Unless the two processes were identical, what matters it what the color of the precipitate was, or whether there was any precipitate at all?

Again, while the chemical experts for the defence were striving to impress on the jury "the interesting and important fact" that they could obtain a precipitate of "a decided orange-red," of "a rich orange-red, which would defy any chemist to distinguish from the (supposed) characteristic orange-red sulphide of antimony without the presence of an atom of antimony," did they know that what they call "a rich orange-red precipitate" was not orange-red at all? Did they know its true color? Did they know it was nearly white? Were they aware that no human eye can determine the true color of a precipitate seen through a colored liquid? It would be too monstrous an assumption to suppose they did know all this at the time their evidence was given. That would have been what the Attorney-General called, in forcible language, "an atrocious fraud upon the administration of justice." Yet they either knew or did not know the facts in this matter. And, as I cannot assume that they did know them, I am compelled to assume that they did not know the facts. In that case, says the Attorney-General, they betrayed "the grossest ignorance." While one need not use such strong language, I cannot help thinking they exhibited great carelessness in not studying more carefully the conditions of the problem they had to solve. It is not at all surprising that one observer

should make a careless experiment, or rest satisfied with an unwarranted deduction. But it seems to me very surprising that "Prof. McCullough's experiments," with all their errors in fact and sophistry in deduction, could have been "repeated and verified by Dr. Genth and by Profs. Reese and White," and yet none of these gentlemen discover the fatal defects here pointed out. It is plain to me that they all overlooked certain essential conditions which could not be neglected without vitiating all subsequent results. The celebrated Dr. Cullen, it is said, was accustomed to arrange all medical facts under two heads, as true facts or as false facts. In which of these two classes we are to place the wonderful discoveries of Prof. McCullough, "verified by Dr. Genth and Profs. Reese and White," can only be satisfactorily determined by reiterated experiments carefully conducted by competent observers. The importance of the subject in the interests of science and of legal medicine and of justice cannot be overrated, and would seem to warrant the expenditure of the time and labor required. I offer the results here embodied toward a solution of the problem, and will trust that, by the aid of others, the matter will soon be decided beyond the possibility of cavil.

Since the foregoing was written another remarkable murder trial has been concluded—the Schoeppe case, in Carlisle, Pennsylvania, exhibiting some striking analogies with the Wharton case. To some extent the same experts were arrayed against each other, and in both the defence relied upon certain assumed possibilities to acquit the prisoner. These possibilities the jury were told ought to be considered as probabilities; and, viewed as probabilities, they were so nearly established truths that they were abundantly sufficient to create doubts, and doubts once established must acquit the prisoner. In both, the writer, as a witness for the prosecution, has been a target for the paper pellets of interested parties, who, endeavoring to be sensational, overlooked decency and propriety. For a very long time, while the result of the Schoeppe trial remained undecided, I carefully abstained from any public discussion of the case, since nothing I could have said could have been of any service to the prisoner in his effort to reverse the first verdict. Having no personal feeling to gratify, nd unwilling to interpose any obstacle in his struggle for

life, I preferred remaining silent while the case was pending. It is now ended, and I propose simply to give a few facts, which, for very obvious reasons, have been so studiously kept out of sight by others, that my connection with the case has been entirely misapprehended. In the Schoeppe case the prosecution proved the presence of prussic acid in the stomach of Miss Stinnecke by tests which could not mislead. These were the production of Prussian blue and the red sulphocyanide of iron. All the experts for the defence admitted on the former trial (and would have been obliged to admit on the last had the question been asked) that no one thing except prussic acid was known, that, treated in the way described, could have given the results Dr. Aikin obtained. Therefore, it became necessary to show that the prussic acid found was formed extemporaneously in the progress of the analysis, and various substances were named that would have produced prussic acid if any of them had been present. But the prosecution proved that the stomach of the deceased was absolutely and perfectly empty: when cut open, no solid matter was found in it, and not a drop of liquid ran out; when held up not a drop of any thing fell from it. Therefore, none of the things supposed could have been present, not even saliva from the mouth, for that was not present in sufficient quantity to form a visible drop, and it is easy to show by a simple arithmetical calculation, based on the admitted chemical composition of saliva, that ten drops could not yield enough prussic acid to be detected by any reliable test. Then the defence claimed that the empty stomach itself would produce prussic acid under the conditions of the analysis, and cited authority to show the possibility of such a result. But the authorities admitted the possibility only when putrefaction had already commenced in the substance of the stomach, and it was in evidence that there was no putrefaction in the stomach of Miss Stinnecke, and therefore the prussic acid found could not have been supplied by the stomach itself under the conditions given by the authorities. The amount of the evidence was then objected to as insufficient, and other tests were cited which it was said ought to have been employed. But if the tests employed gave results which the defence admitted could only have been supplied by prussic acid—that is, if nothing

was known which, treated in the manner described, could have produced the results obtained except prussic acid—then the production of those results proved, beyond the possibility of doubt, that prussic acid was present. How it got there, when was it put there, who put it there, was it the cause of death, were questions which chemical analysis could not answer, and questions with which the analyst was not concerned. His opinion was not asked on these topics, and if it had been he would not have been allowed to express it, and the jury had not the slightest evidence as to what that opinion might be. He had to deal only with the fact that prussic acid was present, and that fact he believed and still believes was established. It was established by proofs that left no shadow of doubt, and could not have been made any more certain by additional proof. The objection that the analysis was not exhaustive, has often been urged in similar cases, and may be answered in the words of an eminent English writer on medical jurisprudence and toxicology. Taylor says: "A criminal is not to be acquitted upon the assumption that a more delicate chemical process might have been adopted by the crown witnesses for the detection of poisons in a dead body, for there is not a criminal case in which an unscrupulous lawyer might not procure this kind of evidence of opinion in favor of the most accomplished professional poisoner. There are various methods of arriving at the same chemical result, and every analyst thinks his own process the best." The analytical chemist, when called upon to aid in the administration of public justice, has a public duty to perform, no matter how distasteful it may prove. He must be prepared to encounter all kinds of abuse, and to find all the worst feelings of human nature arrayed against him. Personal animosity, professional jealousy, the antagonism of rival institutions, national feeling, sectarian prejudice, and maudlin sympathy, will leave nothing undone to destroy his evidence or to destroy his character. One man will object to his length of years, another to his length of beard. It is the same everywhere, and an eminent English chemist has given an instructive record of his personal experience in such matters. The evil may not yet have attained such huge proportions in our country as in England, but it must be manifest to every one that the same agencies at work here will produce the same

evil results already experienced there. When prominent scientific men will testify on the witness-stand that they have not received any thing for their services, that they have been promised nothing, and do not expect to receive any thing, and afterward bring suit to recover large professional fees for the same services, we would seem to be rapidly approximating to the condition of things that Taylor describes. Speaking of the course pursued by the defence in the celebrated Palmer case, he says it "argues but little for the knowledge or moral feelings of medical witnesses, and must shake the confidence of the public, as it has already done to a great extent, in the trustworthiness of medical opinions. Such must be the result when scientific witnesses accept briefs for a defence, when they go into a witness-box believing one thing, and endeavor to lead a jury by their testimony to believe another, when they make themselves advocates and deal in scientific subtleties instead of keeping to the plain truth. Such men should be marked by the public, and their efforts at endeavoring to confer impunity on the foulest crimes, and to procure the acquittal of the most atrocious criminals, should be duly noted. The chemical defend ers of the culprit Tawell on the 'apple-pip' theory, were in the foremost rank to defend the culprit Palmer. Fortunately for society, their efforts did not prove successful in either case. In the mean time this pernicious system is a heavy blow, and a great discouragement to the detection and exposure of murder by secret poisoning. No man in this country can henceforth venture to denounce a grave crime of this kind committed by a person of wealth or of social position without being prepared to incur the most calumnious attacks, and to have his opinions and motives grossly misrepresented. If, after due consideration, he boldly expresses his opinion at an inquest, and persists in it, he is said to be prejudiced; if he hesitates or expresses himself timidly, he is not to be trusted. There is but little protection afforded to a witness by a court of law; the accused person is there the sole object of sympathy and consideration, and a learned counsel is only mildly rebuked who, against the whole bearing of the scientific evidence, asserts that the prisoner is innocent, and asks the jury to adopt his venal assertion in preference to the unbiassed opinions of medical men."